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The Revd. Edward Hawkins, D.D.

With the author's sanction

ON THE

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FŒTUS IN UTERO

AS

INOCULATING THE MATERNAL

WITH THE

PECULIARITIES OF THE PATERNAL ORGANISM;

AND ON THE TRANSMISSION THEREBY OF

SECONDARY OR CONSTITUTIONAL SYPHILIS

FROM THE MALE TO THE FEMALE PARENT.

BY

ALEXANDER HARVEY, M.D.,

Late Physician to the Aberdeen Royal Infirmary; and formerly Lecturer, sometime on the Institutes, and afterwards on the Practice of Medicine in the University of Aberdeen.

(Read before the Medical Society of Southampton, December 2, 1856; and reprinted from the GLASGOW MEDICAL JOURNAL, January, 1859.)

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ON THE FŒTUS IN UTERO

AS

INOCULATING THE MATERNAL WITH THE PECULIARITIES
OF THE PATERNAL ORGANISM, ETC.

BY ALEXANDER HARVEY, M.D.

(Read before the Medical Society of Southampton, December 2, 1856; and reprinted
from the GLASGOW MEDICAL JOURNAL, January, 1859).

It is now several years since first, in a series of papers in the *Edinburgh Monthly Journal of Medical Science*,* and afterwards in a separate pamphlet, dedicated to the Highland and Agricultural Society of Scotland,† I directed attention to a class of facts which seem to indicate that the *fœtus in utero* may, and in fact habitually does, inoculate the female with the constitutional qualities of the male parent. I did not myself suggest the theory of inoculation. It had been advanced a short time before, in the columns of a provincial newspaper,‡ by Mr. James M'Gillivray, a veterinary surgeon in Aberdeenshire. But I may claim the merit of introducing it to the notice of the profession, and giving currency to it among the agricultural body; as well as of imparting to it a more scientific form, and placing it on a broader basis, than had been done by Mr. M'Gillivray.

In both the first and the second of the series of papers referred to, I pointed out the application of this principle to the question—Whether *secondary syphilis* may not thus be transmitted from the male to the female; and it is to this question that I now wish specially to call your attention, premising that my own has

* October, 1849, and October and November, 1850.

† On a Remarkable Effect of Cross-Breeding. Edinburgh, 1851.

‡ The Aberdeen Journal for March 21 and 28, 1849.

recently been recalled to it by two communications on the same subject, published almost simultaneously, but quite independently of each other, both supporting the same views, and both pointing to that principle of inoculation. One of these is in the *Edinburgh Medical Journal* for October last (1856), by Dr. James B. Balfour of Edinburgh; the other is a "Report" of illustrative cases, in the *Medical Times and Gazette* for October 11th, prepared by Mr. Hutchinson, of the Metropolitan Free Hospital, and to which is appended a summary of the principal conclusions arrived at; the report itself forming the groundwork of a paper which was read by him a little while ago to the Hunterian Society of London, and will shortly appear as an original communication in the pages of that periodical.* And my object is to bring together, as far as known to me, the facts that have been ascertained regarding it by these and other writers; and, by presenting those facts under one connected view, to aid in the further investigation of a subject which is as interesting in its physiological, as it is important in its pathological and practical relations.

But (*reculer pour mieux sauter*) it will be expedient, before entering upon it, to lay before you a brief outline of the general doctrine of maternal inoculation through the foetus, as derived from other facts, and resting on other grounds. Such an outline will, I hope, enable you the better to see clearly the bearings of our proper subject, and to appreciate its importance, not merely as a practical question, but as supplying a test or criterion of great delicacy, and of corresponding value, in regard to the whole general doctrine itself.

I. The general fact on which this theory of inoculation is founded is, that the peculiarities of a male animal that has once had fruitful intercourse with a female, of the class *mammalia*, may be more or less clearly discernible in the progeny which that female may subsequently have by other males; or, in other words, that a male animal that has once had such intercourse with a mammalian female may so influence her future offspring begotten by other males, as, to a greater or less extent, to engraft upon them his own distinctive features and his own constitutional qualities; his influence thus reaching to the subsequent progeny, in whose conception he himself has had no share, and his image and superscription, so to speak, being more or less legibly inscribed upon them. Accordingly, if the female be of a *different* breed or species from that male, and have thus borne a cross or hybrid by him, her future offspring, got by males of the *same* breed and species with herself, may yet have more or less the characters of a cross or hybrid. And if it be true, as is maintained by Mr. Orton of Sunderland, that, in the reproduction of the animal species, the male parent imparts mainly to his offspring the parts

* Since published therein—December 20, 1856, and January 10, 1857.

and qualities that are characteristic both of the animal in general, and of the species to which he belongs, as well as of himself individually—to wit, the external structures or the organs of animal life—the brain, nerves, organs of sense, the skin and hair, together with the bones and muscles—and for the most part, therefore, determines the outward character and general appearance of the offspring—it is precisely certain of those parts and characters which may be expected to appear, and which are, in fact, discernible in the subsequent offspring which the female bears to other males.*

1. This general fact was not unknown to the great Haller. It was known even to Becker, who lived long before Haller; both these writers stating, that when a mare has had a mule by an ass, and afterwards a foal by a horse, the foal exhibits traces of the ass.†

It was not, however, till the publication in the *Philosophical Transactions* for the year 1821,‡ of the cases of Lord Morton's mare, and of Mr. Giles' sow, that the subject attracted any particular attention. A young chestnut mare, seven-eighths Arabian, was covered in 1815 by a quagga—a species of wild ass from Africa, and marked somewhat after the manner of the zebra. The mare was covered but once by the quagga, and, after a pregnancy of eleven months and four days, gave birth to a hybrid, which took strongly after the quagga. Subsequently, in 1817, 1818, and 1821, this mare was served by a very fine black Arabian horse, and produced successively three foals, all of which bore unequivocal marks of the quagga; the first, however, in a higher degree than the third. Again, a sow, of the black and white breed (known as Mr. Western's breed), became pregnant by a boar of the wild breed, of a deep chestnut colour. The pigs produced were duly mixed, the colour of the boar being in some of them very prominent. The sow being afterwards put to a boar of the same breed as her own, some of the produce were observed to be stained or marked with the chestnut colour that prevailed in the former litter. And on a subsequent occasion, the boar being still of the same breed as the sow, some of the litter were also slightly marked with the chestnut colour. What adds to the value of the fact now stated is, that, in the course of many years' observation, the breed in question was *never* known to produce progeny having the slightest tinge of the chestnut colour.

* See two papers by Mr. Orton, on "The Physiology of Breeding," in the *Newcastle Chronicle* for March 10, 1854, and November 10, 1854; and a paper by the author, "On the Relative Influence of the Male and Female Parents in the Reproduction of the Animal Species," in the *Edinburgh Monthly Journal* for August, 1854.

† Haller, *Element. Physiol.*, vol. viii., p. 104; Becker, *Physic. Subterr.* Lips., 1703.

‡ Pages 20 and 23.

These two cases were long looked upon as solitary examples of an exceptional phenomenon, for the reference made by Haller and Becker to a precisely analogous case had come to be forgotten; and the phenomenon exemplified by them was long regarded as scarcely, if at all, admitting of a plausible explanation. By some, however, it was suggested that it might be ascribed to a partial fecundation by the quagga and chestnut boar, respectively, of others of the mare's and sow's ova than those actually impregnated by them; or at least to some influence of their seminal fluid on the ova that remained, of a permanent kind, but strictly *local* in its agency, and modifying in some way the development of these ova, when afterwards impregnated by other males. By others, again, the phenomenon was regarded as an instance of that class which come within the category of "mothers' marks," and are popularly ascribed to the agency of the imagination or of the mind on the body, and supposed to be illustrated by a reference to the case recorded in Genesis (chap. xxx.), of Jacob and his peeled rods. Neither the phenomenon itself, however, nor these conjectures regarding it, gave rise to much discussion, or long continued to engage the attention of physiologists. The subject was altogether passed over by Dr. Bostock, in his elaborate *System of Physiology*. It was referred to by Mr. Mayo and by Dr. Alison, in their "*Outlines*," and by Dr. Kirkes, in his "*Hand-book*," of *Physiology*. But a brief and passing allusion to it was all the notice it received.

Meanwhile, instances of a like kind were becoming known to persons engaged in the breeding of horses, cattle, sheep, dogs, and swine; and in 1849 Mr. M'Gillivray adduced, in the paper I have referred to, a collection of cases sufficiently large to show, that the phenomenon exemplified in Lord Morton's mare, or rather in her progeny, is by no means so unique as it was thought to be. And it may now be confidently affirmed, that since then, and within these few years, enough has transpired in this department of inquiry to warrant the presumption, that the phenomenon in question is so uniform in its occurrence, whenever the requisite conditions obtain, as to constitute a general fact or law of nature. It is so regarded by a large number of our great breeders of cattle, by dog-fanciers, and others of large experience in the rearing of horses, sheep, &c., and is habitually taken into account by them in the selection of their breeding-stock, and in their estimate of the purity of an animal's blood; the *practical* belief of all of them on this subject being, that a female animal that has once been impregnated by a male of a different breed or species from her own, and has borne a cross or a hybrid by him, is thereby *destroyed* for a time at least, if not permanently, for the purposes of breeding *pure* stock of her own kind; and the *theoretical* belief of many among them, that this result happens because (as was first suggested by M. M'Gillivray) the blood of the female has,

through the foetus, been *contaminated* by the blood, and charged with the qualities of the male she first had intercourse with—so charged therewith, so contaminated thereby, that she herself imparts the blood and the qualities of this male to the progeny she afterwards has by other males.

2. It is not my intention to lay before you any additional examples of this kind as occurring in the lower animals. I have elsewhere brought together a tolerably large collection, and to this it must suffice to refer. But I cannot forbear just briefly alluding to the fact, for the mention of which you will be quite prepared, and the reference to which will naturally pave the way to our proper subject—that instances of the like sort are, perhaps, equally common in our own species. It has long been known—and it is, in fact, a popular observation—that, in the case of a woman twice married, and fruitful by both husbands, the children of the second marriage often resemble their mother's first husband or his family, and that not in features only, but in mind also, and in disposition. It is obvious, indeed, that in any such case, where all the individuals concerned—the woman, her children, and both the husbands—are of the *same* variety of the human family, the alleged resemblance must often be exceedingly difficult to trace or to substantiate. But it is equally obvious, that means exist for ascertaining clearly whether it obtains or not. There are equally distinct varieties of the human species as there are of any of the lower animals; and all that is requisite for bringing the question to a decisive issue is, to observe accurately whether the children of European parents—when the mother has, in the first instance, had offspring by a negro—exhibit traces of the latter in the colour of the skin, and still more in the quality of the hair, the form of the features, &c.; or, contrariwise, whether the children of negro parents, when the mother had formerly been impregnated by a European, manifest the peculiarities of the latter.

Of the former case, I have heard of two instances as occurring in this country, one of them under the notice of Professor Simpson of Edinburgh:—A young woman, born of white parents, but whose mother had sometime before her marriage had a bastard child (a mulatto) by a negro man-servant in Edinburgh, presented distinct traces of the negro. Of the latter case, that in which the parents are both negroes, but in which the mother has previously had fruitful connection with a European, Dr. Robert Balfour, of Surinam, wrote me some years ago to say, that repeated instances of the European influence under such circumstances, had come under his own observation.* Doubtless, if looked for, examples

* "Among the coloured population" (Dr. Balfour writes me, April 19, 1851), "consisting of negroes and their offspring by Europeans, the most striking illustrations of the truth of your or Mr. M^cGillivray's theory are constantly occurring. Shortly after my settlement here, I was led to remark the circumstance (which I had never noticed recorded by writers), that if a negress had a

of the influence now in question would be found as general in our own species as in the lower animals. And it may just be observed that, in the one as in the other, they can only be unequivocally and satisfactorily determined in cases where the individuals or the animals that are the subjects of observation are of a different race or species; or rather, where the male, the female, and the offspring, are of the same breed and species, but the female has first of all been impregnated by a male of a different breed or species.

3. In ascribing this very remarkable phenomenon, as seen both in our own species and in the lower animals of the class Mammalia, to an agency exerted by the foetus on the mother's system, of the nature of inoculation, I do not intend to consider in detail, or in connection with other explanations that have been given of it, the grounds on which that doctrine of inoculation rests. The questions involved in it are too numerous, and the discussion of them would be too tedious, to admit of my entering upon them at present. I have elsewhere considered them pretty fully, and must here content myself with this general remark, that the facts there advanced in support of the theory of inoculation have always seemed to me sufficient, not indeed absolutely to prove the truth of it, but to impart to it a probability short only of absolute certainty; and, further, that while I deemed it expedient to suggest to the agricultural body certain experiments in breeding, which, if carefully made, could not fail to be decisive of it, I have always thought that a sufficiently large collection of facts, of the description of those now to be laid before you in regard to the poison of syphilis, would supply all that is really wanting to make the *demonstration* of its truth complete. The *reasonableness* of the doctrine no one can well dispute. Connected as the foetus and its mother are by the placenta, a mutual interchange of fluid matters is continually going on between them; the foetus drawing supplies from its mother's blood for its growth and maintenance, and the mother (though, no doubt, in much less quantity) abstracting materials from the blood of the foetus. Is it unreasonable to suppose that the matters abstracted by the mother may be charged with (or have inherent in them) the *constitutional* qualities of the foetus; or that passing into her body, and mingling there with the general mass of her blood, they may impart those qualities to her system? And this supposition will perhaps appear the less improbable, if regard be had to the length of time during which the connection between the mother and the foetus is kept up, and during which this transference of materials must go on—a period of some weeks, or even of several months. But the qualities inherent in the foetus must in part be derived by it from its male

child or children by a white, and afterwards fruitful intercourse with a negro, the latter offspring had generally a lighter colour than the parents. . . . I could particularize many such, the observation of which had led me to form an opinion similar to that which you have more elaborately worked out."

parent, and be to that extent identical with his. The distinctive peculiarities, therefore, of this parent may thus come to be *engrafted* on the mother, or to attach in some way to her system; and if so, what more likely than that they should be communicated by her to any offspring she may afterwards have by other males?

4. Hitherto we have had under consideration the transmission of such qualities only as are strictly normal or healthy, and so far the subject may be regarded as one of physiological interest merely, or at least of practical importance to breeders alone. But it is conceivable that *morbid* qualities may be transmitted also, and be in the same way, or through the same channel, engrafted on the mother's system. In our own species, scrofula may, peradventure, be thus transmitted. In my first paper on this subject, I not only refer to the possibility of this, but give two instances in which it apparently happened.* A serious difficulty, indeed, attaches to the investigation of this point. So *widely* diffused is the scrofulous diathesis among mankind, that, in any single case in which it has apparently been acquired in this way, it would be difficult to determine whether it was *really* thus derived or not. Still, observations on the large scale, long and carefully conducted, as to the health and constitutional tendencies of females before and after marriage, as compared with those of their husbands and children, and carefully distinguishing between females that have, and females that have not had children, might in the end, and after full allowance for errors or fallacies of observation, sufficiently demonstrate the reality of the occurrence. Certainly it is not an uncommon observation, that women born of healthy parents, and themselves previously healthy, fall into a state of general ill-health after becoming mothers†—an observation which may yet be connected, in a certain proportion of cases, with the principle now in question. So, likewise, may the syphilitic poison, in its *constitutional* forms, be transmitted after the same manner from the husband to the wife, and be by her conveyed to the children she may subsequently bear to a second husband, himself perfectly free of all syphilitic taint. Whether any *other* morbid states

* Edinburgh Monthly Journal, Oct., 1849.—The one, reported to me by Dr George Ogilvie of Aberdeen, of a woman who was twice married, and had children by both husbands, and whose children by both marriages were scrofulous—although only the first husband had marks of that diathesis—the woman herself and her second husband being, to all appearance, quite healthy;—the other, by Professor Pirie, and as follows:—Mrs. H —, apparently perfectly free from scrofula, married a man who died of phthisis. She had one child by him, which also died of phthisis. She next married a man who was seemingly equally healthy as herself, and had two children by him, one of which died of phthisis, the other of tubercular mesenteric disease, having at the same time scrofulous ulceration of one of the lower extremities.

† Even this observation has not escaped Mr. Kingsley. Speaking of Mrs. Vavasour, he says—"And when children, and the *weak health* (on the part of the mother) which comes with them," &c.—*Two Years Ago*, vol. i., p. 70.

besides these—*e. g.*, mania, epilepsy, gout, &c., which are unquestionably hereditary—may thus be imparted and transmitted, it is at present impossible to say. Enough appears, however, from a consideration of the transmission of healthy qualities, and from what is known in regard to the transmission of syphilis, to impart to the whole inquiry, as to the transmission of morbid qualities, an interest of the highest practical kind, and that in a *social* as well as in a purely *professional* point of view. So important may it yet be ascertained to be, in contemplating marriage with a widow, to have regard to the constitutional peculiarities of the deceased husband!

So much for the general outline I spoke of at the outset—fuller, indeed, than was intended, but, perhaps, not more so than it was necessary to premise in order to the right understanding of our proper subject—the foetus in utero, as the channel of transmission of secondary or constitutional syphilis from the male to the female parent.

II. Taking up, now, the subject of *Syphilis*, in connection with the alleged inoculating power of the foetus, let me just observe at the outset, that there are two points of view from which it may be regarded—two objects with which its investigation may be conducted:—*First*, As a part of the history of syphilis, and in relation to pathological and practical ends; and, *secondly*, as a test or criterion of the general doctrine of inoculation itself, its special value as a test lying in this, that nothing like *mental* influence can possibly be imagined to have any share in the transmission of the virus. And these two objects, although so far distinct, need not—nay, cannot be separated, in the prosecution of our scientific inquiries. That is to say, if such inquiry shall demonstrate that the syphilitic poison in its constitutional form may be, and actually is, transmitted—nay, is only communicated, and is communicable only, from the husband to the wife through the foetus—while this fact would form a real and practically important addition to our knowledge of the laws of that poison, it would, at the same time, establish the general doctrine of inoculation, as exemplified in other modes, and resting on other grounds; and it would impart, besides, a peculiar strength to the evidence in support of it thus derived, because absolutely devoid of all leaven of the fallacy which may be supposed to attach to that and every other source of evidence.

1. The *first* question I shall consider, as more obviously connecting itself with the general features of the cases already referred to, and exemplified in Lord Morton's mare, is—Whether a woman, twice married—first to a constitutionally syphilitic husband, next to a husband untainted by that poison—and fruitful by both, may herself (not otherwise tainted than through her former husband) infect the children of her second marriage?

In seeking formerly (in 1849) for a solution of this question,

which I thought I had been the first to suggest, I was unable to do more than adduce in illustration of it an imperfect observation from Messrs. Maunsell and Evanson, to the effect, "that they have notes of the case of a syphilitic child, whose mother had been infected by a former husband, and to all appearance cured five years before its birth; the father of the child, her second husband, being in perfect health."* They do not say, however, in what way she was infected; and as it may have been in that of primary affection or chancre, the case, as it stands, is of no value in relation to the question before us.

Dr. Montgomery, of Dublin, I afterwards found had been beforehand with me in this question as to syphilis. Referring to Lord Morton's mare, and to Mr. Giles' sow, Dr. Montgomery remarked, in 1837, in the first edition of his great work "On the Signs and Symptoms of Pregnancy:—

"Such occurrences appear forcibly to suggest a question, the correct solution of which would be of immense importance in the history and treatment of disease. Is it possible (he asks) that a morbid taint, such as that of syphilis, for instance, having been once communicated to the system of the female" by a conception, "may long linger there, and influencing several ova, continue to manifest itself in the offspring of subsequent conceptions, where impregnation has been effected by a perfectly healthy man, and the system of the mother appearing to be at the time, and for a considerable period previously, quite free from the disease? My belief" (he adds) "is certainly in favour of the affirmative."

What Dr. Montgomery here suggested as a probable occurrence, and then merely suspected, he has since been able to substantiate as a fact:—

"Such" (he observes in the second edition of that work, recently published) "was the opinion I expressed in 1837; and further experience and observation have, I think, shown to be a fact, what I could then only venture to say I believed to be likely."

He then proceeds to quote two cases in support of this observation—one from M. Vidal, the other from M. Cazenave—remarking afterwards that, while he thought the observation of the fact was original with himself, he had found it was made long ago by others—by M. Gardien, in his *Traité d'Accouch.*,† and by Dr. F. H. Ramsbotham, in the *Medical Gazette* for May 23, 1835.‡

* On the Management and Diseases of Children, 5th edit., pp. 452, 453.

† Edition 1824, vol. ii., p. 29.

‡ We shall see by and by how clearly Dr. Ramsbotham had divined it. It seems to me, however, very doubtful whether M. Gardien had any conception of it whatever, judging at least from the passage referred to by Dr. Montgomery, and which is as follows:—"Le moment de la conception devient quelquefois une circonstance qui favorise chez la femme le développement de differens virus qui étaient restés sans action jusqu'à ce moment. L'état de grossesse manifeste quelquefois des symptomes de virus vénérien qu'on n'avait pas encore aperçu, et qui, sans cela, aurait pu rester encore plus ou moins de temps engourdi. Le virus syphilitique, avant la conception, pouvait circuler dans la masse generale

In the case given by Cazenave,* the syphilitic taint was transmitted by the mother to the children of her *third* husband, as well as to those of her second; but the case is essentially defective, inasmuch as it is not stated in what manner the woman *originally* became affected. It would even appear from the narrative of the case, that it was while a widow that she became affected; and as it does not appear that she had a child subsequently till she married her second husband, it is probable that she acquired the disease in the primary form, at least otherwise than through impregnation. M. Vidal's case, however, is in every respect unexceptionable, and exactly to the point:—

"A woman, whose husband was affected with constitutional syphilis, gave birth to a child, which in two months showed symptoms of that disease, of which it died. The woman never had any appearance of syphilitic affection, not even sufficient to soil her linen. Her husband died, and, after remaining some time a widow, she married a healthy man; and about twenty months afterwards, being four years after the former birth, she bore a child, which in two months presented the same form of syphilitic eruption which had appeared on the former child."

It is plain that opportunities for observing cases of this description must be comparatively very rare. A second marriage, indeed, on the part of widows, is not uncommon; but with the conditions requisite for thus tracing the transmission of the syphilitic poison, very uncommon. And it must be kept in mind, that in some cases of *this* kind, the results of observation (although, as we shall see, of great value notwithstanding) will be of a *negative* character, as in the following case given by Dr. James B. Balfour:—

"A respectable young woman from the north was married to a tradesman, who had no trace of syphilitic disease at the period of marriage; but he afterwards acknowledged that, two years before, he had the disease, followed by slight secondary symptoms, which had entirely disappeared under medical treatment, and he had seen or felt nothing since. This woman complained of nothing until about three months after she became pregnant. Then, however, symptoms of secondary syphilis became apparent—spots of psoriasis appeared on various parts of the body—hard knots were felt on the perineum, and on the external labiæ—and within the vagina there was a hard, knotty feeling over the whole mucous surface. Her child exhibited a distinct syphilitic appearance, which was removed by the usual treatment. Shortly after delivery all symptoms of syphilis entirely disappeared; and as she shortly afterwards removed to the country, she was subjected to no medical treatment. A few months afterwards her husband died. She subsequently married a farmer. About six or eight months since I saw her, and she has borne three children to her second husband, and certainly more healthy children I could not wish to see. She informed me that she had

sans affecter aucune glande, et sans donner de marques de sa présence; parceque ces organes n'étaient pas sensibles à son action. La grossesse augmentant la sensibilité, manifeste des symptômes d'une infection générale, parceque les organes glanduleux ressentent, à cette époque, les impressions du virus vénérien, auquel ils étaient insensibles auparavant."

* *Traité des Syphilides*, etc., p. 133.

never been under medical treatment since I had attended her—indeed, she never had a medical man near her, except during her confinements—and that she had never suffered, during any of her pregnancies, from anything like what she had done at the first one.”

I shall not trouble you again with the details of individual cases. But I have thought it desirable to quote at length the two I have taken from Dr. Balfour and M. Vidal, as being fair examples of the kind of cases we may expect to meet with, and on which our conclusions must be founded; few of them exactly tallying in every point; most of them having each its own special features, and the comparative value of the several cases being widely different—none, however, being of any value in which it does not clearly appear that the virus was imparted to the female in its secondary or constitutional form only. In both the cases adduced this essential condition was plainly apparent, but they differed in other respects. In M. Vidal's, the woman never exhibited in her own person any symptoms of syphilis, but gave proof of being tainted with it by giving birth to a syphilitic child, got by her second husband, a healthy man. In Dr. Balfour's this evidence was wanting; but the fact of the woman having imbibed the poison from her first husband, appears from the symptoms of it having shown themselves upon herself during her first pregnancy; the evidence thus afforded deriving confirmation from this other fact, that the child she then bore was also affected with it.

2. And this, for the most part, is the *sort* of proof we should look for—direct evidence furnished by positive manifestations in the person of the mother, and confirmed by like manifestations in that of the child, of syphilitic taint. Although always interesting, and indeed important to do so, it will not actually be necessary, except in a very few cases (as in M. Vidal's), to seek to trace the transmission of the virus to the children of a second marriage, and of a healthy father. The proof we require lies within a narrower compass, and may be reached by a shorter process. Dr. Balfour's case, rather than M. Vidal's, is the type of the cases we have to search for and to consider; and, fortunately for the investigation, they are of much more frequent occurrence. And the question to be considered, in the first instance, is simply this—Whether ever a woman derives secondary syphilis from her husband *when* she conceives by him, and *because* of her doing so.

The evidence now available on this point, as far as known to me, is of two kinds:—*First*, General statements of experience supplied by M. Ricord, Dr. Montgomery, Dr. Tyler Smith, Dr. Carpenter, and likewise by the author of the review, in the *Edinburgh Medical Journal*, of the new edition of Dr. Montgomery's work, and by Dr. Ramsbotham; and, *secondly*, Specific instances observed and recorded by various writers.

Let us first consider the former branch of the evidence. M.

Ricord, one of the highest authorities on all points relating to syphilis, observes that his experience goes to prove, that in the case of a woman pregnant of a child whose blood is contaminated with syphilis acquired from the father, this child may, and often actually does, contaminate the mother's system:—

"So long as a diseased father (says M. Ricord) is under the influence of constitutional syphilis, the germ which is by him conveyed into the uterus carries along with it the syphilitic diathesis. . . . There is no such thing as an infection of the child by the mother, she having been contaminated by the father; but the husband procreates an infected child, which may then propagate the secondary poison to the mother—for where there are no children, the mother does not suffer."*

Dr. Montgomery, in the last edition of his "Signs and Symptoms of Pregnancy," speaking of this very point, remarks, that "there can be no doubt of the *frequent* occurrence of the fact." Dr. Tyler Smith's testimony is, that the most common mode in which women become affected with syphilitic *uterine* disorder, is, he believes, that in which the *fœtus* is the medium of communication.† Dr. Carpenter, speaking of the inoculation theory, says—

"This idea is borne out by a great number of important facts; and it serves to explain the circumstance, *well known* to practitioners, that secondary syphilis will *often* appear in a female during gestation or after parturition, who has never had primary symptoms, whilst the father of the child shows no recent syphilitic disorder."‡

And the reviewer referred to observes—

"That a woman may get syphilis from a man without any external disease—namely, by bearing a syphilitic child in the womb of her previously healthy body, which child infects her with the disease; and that she may then exhibit outward signs of the disease now in her constitution—or, without this, may prove her syphilitic taint, by producing syphilitic children to another perfectly healthy father."

Let us see, further, on this branch of the evidence, what Dr. Ramsbotham says. Among the first, if not *the* first, to suggest the possibility—nay, the great *probability*—of this mode of transmission of the syphilitic poison, nothing can be clearer or more explicit than his observations on it:—

"It is a commonly received opinion (says Dr. R.) that syphilis, in its secondary stage, is not communicable directly to either sex from the other—that the disease is not propagated unless there exist an open chancre, and this accords with my observation. But it appears to me *probable*, that if a previously healthy woman conceive of an ovum tainted by syphilitic virus derived from its father, her system may become *inoculated* during the progress of gestation, in consequence of the close vascular connection existing between it and herself; for it has fallen to my lot to see *more than one case*, in which a young woman, united

* The Lancet, April 8, 1848, p. 383. See also Mr. Acton's work on Syphilis, p. 632.

† See Association Medical Journal, July 14, 1854.

‡ Principles of Human Physiology, 5th edition, p. 826.

to a man labouring under obstinate secondary symptoms, remained *healthy for some months after marriage*, but became the subject of the same disease in its secondary form, *soon after impregnation had taken place*; and *I have considered that, in such a case, the mother derived the disease, not directly from the father, but from the affected infant which she carried in her womb.*"—(*Loc. Cit.*)

These are strong and unequivocal statements, and the specific evidence furnished by individual cases is equally decided. To what extent the records of medicine can now supply examples of this description, I cannot say. But I have already repeatedly alluded to those brought forward by Dr. Balfour and by Mr. Hutchinson; and it is to these that I would now particularly refer. The former gives four or five cases, the latter as many as fifty. To be duly appreciated, these cases must be examined individually and compared together—a task which it were foreign to my object to enter upon at present, even were it possible to accomplish it, without actually reproducing the cases in detail. It must suffice, therefore, thus to refer to them, and to remark that, having carefully considered them, I think they conclusively establish in the affirmative the question presently before us; proving that a woman may, and often actually does derive syphilis from her husband, when she conceives by him, and in consequence of her doing so.

3. The next question to be considered is—Whether at each pregnancy there may be a *renewal* of the symptoms of secondary syphilis in the woman—implying that she has from some source, or from some cause connected with the state of utero-gestation, imbibed a fresh dose of poison?

On this point, and speaking apparently of uterine syphilitic affection, Dr. Tyler Smith stated at a meeting of the Medico-Chirurgical Society, that "he had observed in such cases, that at each pregnancy a fresh dose of the syphilitic poison is imparted to the mother, unless in the meantime the husband had been the subject of anti-syphilitic treatment."* Mr. Hutchinson's experience as regards the ordinary secondary symptoms is to the same effect. Our time will not allow us to refer specially to his cases, as bearing on this question; but it will suffice to quote his own general conclusion:—"Increase of symptoms and relapses may be produced by repetition of exposure to contagion"—*i. e.*, by the woman again becoming pregnant. And in one of Dr. Balfour's cases, the first in his collection, there was a recurrence of the syphilitic symptoms during the second pregnancy.

The positive value of the fact thus clearly ascertained, and still more, its value in relation to other facts, and as checking or testing these, I am disposed to rate very high.† The consideration

* See Association Medical Journal for July 14, 1854.

† As this point is important, I hope I may be excused, if, in order to bring it fully into view, I adduce here so much of Dr. Balfour's first case—the only one in which relapse occurred—as is illustrative of it:—"By the time the lady

of it, however, in this view, must be reserved till another question yet to be proposed comes before us. Nevertheless, to meet an exception that may be taken to it, it will be necessary at this stage to point out how the fact really stands. The reappearance of the syphilitic symptoms in the mother during a second or subsequent pregnancy, seems certainly to imply that she has received a fresh dose of the virus, and likewise that she has received it directly from and through the foetus. But it does not absolutely follow that in every such instance the foetus derives the virus from the father. In most instances it probably does. M. Vidal's case, however, is sufficient to show that it may derive it from the mother exclusively; and, in as far as it does, the reinfection of the mother comes remotely from herself. But this does not alter the character of the fact, or impair its value. Why, while long previously dormant in her system, the virus should thus again become active and reappear, we shall afterwards consider; merely remarking at present that the like question may be raised, and admits of a like solution, in regard to the power of the virus when long dormant in the father's system to infect the child.

4. Another question still remains — Whether ever a woman derives secondary syphilis from her husband *unless* she conceives by him? And the question to which this stands opposed is— Whether she may not derive the virus through the *seminal fluid* also, and indifferently *at any time*?

This question, which was suggested to me by Mr. Paget, as it was, I believe, to him by the perusal of my own observations on this subject in my first paper, is perhaps the most important of all.* Previously, however, to taking it up, the consideration

recovered from her confinement, and the lochial discharge had ceased, all trace of the syphilitic affection had entirely left, and she was so completely restored to health that I did not deem any medical interference necessary. She enjoyed excellent health, and made a good nurse, and she continued quite free from any return of the disease *for more than fifteen months*, when she again became pregnant. About two months after this occurred, a train of symptoms precisely similar to the first appeared, and continued as formerly up to the time of her confinement, when they again disappeared, as on the former occasion. The second child was also syphilitic." It cannot well be supposed that during the interval between the two pregnancies, no sexual intercourse obtained between this lady and her husband till just previously to the second; and that the relapse and the reimpregnation were merely coincident.

* The alternative question might have been framed in more general terms so as to include tainted *proputial* as well as *urethral mucous secretions*, together, with exudations from vesicular or pustular eruptions on the penis. But I have restricted it to the seminal fluid in accordance with Mr. Paget's communication. "I would venture to suggest," Mr. Paget wrote me, "that you should try to find whether ever a woman derives secondary syphilis from her husband *unless she conceives by him*. Facts bearing on this point might prove that secondary syphilis is not communicated directly by the *seminal fluid*, but by the child begotten with it; and this mode of inoculation being proved, would go far to prove the foundation of your theory."

already referred to, as to the power of a seemingly *dormant* virus, demands attention. And it does so as enabling us to understand why—if the fact be so—it is through the *fœtus* only, and during pregnancy alone, that the female derives the virus from her husband, or, in a few cases, virtually reinfects herself; and, likewise, in the absence of positive proof, and until such proof is adduced, that she may and does, making it *probable* that she will not and cannot derive it through the seminal fluid, and indifferently at any time.

Now, although in many cases, in the lower ranks of life particularly, the syphilitic symptoms in the husband may continue to show themselves, and be severe or well-marked even after marriage, the general fact, I apprehend, is, that among all classes, and certainly in the higher, they have long previously disappeared; that in a large proportion of the cases they were originally mild, or very slight in the later periods of their manifestation; and, further, that, to all appearance, the virus itself has been completely eradicated from the system. Facts, indeed, clearly demonstrate that it must still be present there. But facts demonstrate, also, that it must be present in such quantity and in such condition as to be incapable of again exciting actual disease in *him*. And the question arises—how, under such circumstances, should the virus be capable of tainting and sensibly affecting the *child* begotten by him? The answer is not far to seek. In accordance with the known laws both of physiology and of the morbid poisons, the *fœtus*, being an entirely new formation, and the seat and subject of nutritive changes of great activity, will be *peculiarly susceptible* of the virus; and the virus itself, although existing in the father's system, and imparted by him to the *fœtus* in a quantity which may be infinitesimally small, acting as a *ferment*, multiplies itself in the blood of the *fœtus*, and at length *accumulates* there in such quantity, acquiring at the same time such *efficiency*, as to produce in it *manifest* syphilitic disease. The like explanation will apply to the cases in which (as in M. Vidal's), the virus is derived by the *fœtus* from the mother. And that the *fœtus* thus contaminated, whether by the mother or the father, and thus actually affected by the poison, should readily infect or reinfect the mother, is only what might reasonably be expected. Thus, strictly speaking, it is not the father that *directly* affects the mother. It is the *fœtus* that does so. The virus, in passing from the father to the mother, passes first into the system of the *fœtus*, and there multiplying, acquires the requisite power. And it may be, and I confess I cannot but believe that it is *thus* only that, while *latent* in the system of the husband, or appearing only in the *slight* and *mild* tertiary form, it ever *will* or *can* affect his wife.

To revert now to the questions put a little ago. We have already seen that the female may, and often does derive syphilis

from her husband when she conceives by him, and that—whether remotely derived now from herself or not—she may, and often does *again* exhibit the like symptoms on a subsequent impregnation. But may she not also do so independently of conception, and at any time? May not sexual intercourse alone, and the mere application of the seminal fluid, or of tainted preputial secretions, suffice to impart the virus to her? I am inclined to think not. It were a rash assertion to say that its occurrence is impossible, and one at least which it would require a large amount of negative evidence to make good. We know that a syphilitic infant may infect its nurse, and it is clear that the infection must come to her through her own nipple and the infant's mouth—abrasion or fissure of the nipple being probably a condition essential to the result. In this case, however, we have an infant labouring under *actual* syphilitic disease; and if infection be possible in the case of man and wife without impregnation, I apprehend it is so only in cases in which the man is at the time *actually diseased*, and of these, probably in those only, in which the female is labouring under ulceration or abrasion of the os uteri. But that it is possible in cases in which the syphilitic affection in the man is altogether latent, or shows itself in its slighter forms, and the mucous surfaces in the woman are entire—I do not believe. Yet it is in cases of this description that it is so often seen in connection with pregnancy—the woman healthy, and the man without any ostensible syphilitic affection.

The question, however, is one of fact. A single well authenticated and unequivocal instance of infection occurring independently of conception, would establish the infecting power of the seminal fluid; or else of certain others of the male genital secretions. Have any such cases ever been met with and recorded? Dr. Balfour refers to none, and seems to consider it certain “that the mere fact of coitus between the woman and her husband is not sufficient to produce the disease.” Mr. Hutchinson states that he has met with but one case of this class; but he regards it as a doubtful one, and he holds it to be “extremely doubtful if ‘contagion by the seminal fluid’ be possible”—observing “that cases are extremely rare in which married women who have never conceived, become the subjects of constitutional taint without having had primary symptoms, and are in all probability to be explained as errors of observation.” And, as we have seen, M. Ricord states emphatically, that “when there are no children the mother does not suffer.” I have not myself met with any, although for nearly eight years I have had my attention directed to cases of this description; nor have I been able in the course of my reading to find any described. It is true that, writing in 1850, I said—“I fear it will turn out on inquiry, that secondary syphilis may be transmitted directly by the seminal fluid, independently

of conception.”* But this apprehension was founded, contrary to my hope, on answers I received to inquiries made of some professional friends of experience in this department of practice, who *thought* they had seen cases in which the female had been tainted by her husband, and (the idea of inoculation through the foetus being new to them, and appearing strange) were persuaded that the contamination *must needs* have occurred through the seminal fluid. To the apprehension, however, then expressed, I appended this remark:—“but perhaps it may appear also that its transmission in this way (*i. e.*, by the seminal fluid) is *occasional* only and *uncertain*, while it is *very frequent*, or almost *inevitable*, when conception follows intercourse”—adding, that “a comparative observation of this kind, if clear and undoubted, would be nearly equally decisive.”

And such I think is the ground we should still take, and the spirit in which our investigations should still be carried on:—An admission, on the one hand, that inoculation by the seminal fluid, or otherwise than through conception, is not *so* impossible in certain cases as to prevent us carefully watching those in which, if it be possible, it may peradventure occur, or make us absolutely to reject all evidence which may be offered in support of the transmission of the virus in that way; and, on the other hand, a reference of the whole question at issue to trial by *comparative* observation so soon as a sufficiently large number of observations have been made. And I think we are now in a position, and that mainly through the labours of Mr. Hutchinson and of Dr. Balfour, to submit the question to this issue. The inquiry is still in its infancy; but the facts already ascertained appear to be sufficiently numerous to warrant our drawing certain inferences from them, and, in particular, the following:—*first*, that if inoculation, independently of conception, be possible, it at least occurs very rarely—so rarely as to make it doubtful whether it ever does, and, further, that no proof yet exists to show that it ever has occurred; and, *secondly*, that inoculation by the foetus—never suspected till within these few years, and by many still thought improbable—is not only possible, but is at once the mode in which it almost invariably (at least) occurs, and of very frequent occurrence besides.

Nothing, I think, can now set aside these conclusions. Further observations may modify them; but the evidence on which they rest is large enough and clear enough to enable us confidently to rely upon them. And its strength lies not merely in the number of instances in which different females have become infected with secondary syphilis when they conceived, *and then only*, but in which the same female has been reinfected simultaneously with

* Monthly Journal of Medical Science for October, p. 303.

reimpregnation, and at no other time—continuing free from syphilitic affection for months together, although cohabiting with the tainted husband, and then suffering again on again becoming pregnant. It is this which makes the evidence not merely convincing, but irresistibly conclusive.

III. This properly concludes what I have to say on the *fœtus in utero* as the channel of transmission of secondary or constitutional syphilis from the male to the female parent. Practically important on its own account as the subject is, it is physiologically important also, as a test or criterion of the general doctrine of inoculation through the *fœtus*, as originally suggested by Mr. M'Gillivray. Did our time permit, I would endeavour to point out its bearing in this relation, and its peculiar value. I would proceed to show you that there probably are in nature two sets of cases, or two classes of facts connected with the peculiarities seen in the offspring of mammalia, *analogous*, nay, *identical* in their *external* characters, but widely different in their origin and in the conditions of their production; the one referable to *mental* states in one or other of the parents, but oftenest to mental states in the female, and operating on her either at the time of conception or during the early stage of pregnancy;* the other to a change effected in the constitutional and reproductive powers of the female by a *physical* agency, originally extrinsic to her, but inherent in a former *fœtus* by derivation from its male progenitor, and conveyed to her in the way of inoculation by that *fœtus* while *in utero*.

To establish this, and to demonstrate also the relative proportion in which the two sets of cases obtain, together with the comparative efficacy of their respective causes, and the conditions that are essential to the action of each, would be an achievement in physiology of great general interest; and, in relation to the practice of medicine on the one hand, and the breeding of stock on the other, of real practical importance.

But, in the investigation of one of the branches of this subject, the first and essential point is to make good the law or principle

* Ex. gr.—“A mare and a horse (a gelding) had for some years worked together on the same farm, occupied adjacent stalls in the same stable, and pastured together in summer in the same fields. The gelding was of a black colour, with white legs and face, and had a singular peculiarity in the form of the hind legs, which, when the animal was standing, appeared as if quite straight, there being no appearance of the leg being bent at the hough-joint, as in ordinary cases; the pasterns, likewise, were very long, so as to cause the feet to look as if placed almost at right angles to the legs. After having been some years thus associated with this gelding, the mare was covered by a stallion of the same colour with herself—both stallion and mare being of a bay colour, with black legs and a small spot of white only on the forehead. The foal, which was the produce of this connection, very exactly resembled the gelding in *colour* and in *shape*; and very remarkably in the shape of the hind legs, as above described.”—On a Remarkable Effect of Cross-Breeding, p. 13.

on which it rests—to be well assured that the foundation is secure. And it is in this point of view that the inquiry as to syphilis is so important. As regards *it*, there can be no room for suspecting that *mental* causes can possibly be concerned in the transmission of the virus from the father to the mother. A child born into the world with unmistakeable manifestations of syphilis on its body, begotten by a healthy man, but the fruit of a second marriage, and the first husband and the children of this marriage syphilitic also, could not for a moment be supposed to have come by it *per fortem imaginationem* of its mother, as it might be, if, instead of syphilis, it had the nose or the features of its mother's former husband. As little likely is the mother, by such an effort of mind at the moment of conception, thus to contaminate herself.*

Our time, however, will not allow me to do more than to indicate in this brief manner the value of the syphilitic test of the inoculation *per fœtum* theory; and to add, that, while it seems to me to establish it beyond the reach of all doubt or cavil—the great extent to which recent observations have shown the general fact exemplified in Lord Morton's mare to hold among cattle, sheep, dogs, horses, and others of our domestic animals, as compared with the limited extent to which mental causes are seen to influence the development of offspring, sufficiently demonstrates that in *the great majority* of cases of this description, the *inoculation* principle is that concerned in their production.

IV. There is yet another test of this inoculation principle, for the suggestion of which, as of the former, I am indebted to the sagacity of Mr. Paget. The phenomenon exemplified in Lord Morton's mare *ought*, by the theory, to be *restricted* to animals of the class mammalia, that is, to animals in which the young are developed within the body of the mother, and between which and the mother there exists a placental connection during the whole period of intra-uterine life. But is it actually so restricted? May it not be seen in *birds*, in which the young are wholly developed outside the mother's body, and between which and the mother there is no placental connection?

Now, among bird fanciers crossing is perhaps as extensively practised as it is with our domestic quadrupeds. Nothing, for example, is more common than to breed the barn-door hen with the bantam cock one year, and the next year with a cock of her

* There seems to be with some as little limit to the physiological agency of the imagination as to their own exercise of this faculty. In a case of legitimacy that came before it, the parliament of Grenoble went as far as to declare the issue legitimate, although the husband had been four years absent from his wife, on the ground of an allegation by the latter, that she had conceived through the power of the imagination alone:—"Ut mulier per fortem imaginationem putaverit se in somnis rem habuisse cum marito, atque sic concepisse." Had the child, however, been born syphilitic, it is possible that the parliament might have come to a different decision—straining at a gnat, albeit making no difficulty of swallowing a camel.

own kind ; or the hen-canary one year with either the cock-linnet or the cock-goldfinch, and the next with the cock-canary. In these and other cases of the like kind, cross birds or mules are obtained the first year ; and these, in accordance with the law laid down by Mr. Orton of Sunderland, take very strongly after the cock. But are the birds obtained the following year, when the parent birds are both of the same species and of the same breed, perfectly *pure*, that is, without taint or trace of any of the characteristics of the male bird with which the hen was bred from the previous year ?

On this point I made very extensive inquiry several years ago in quarters where information was to be had, carefully concealing the object I had in view in making the inquiry ; and the uniform result was, as I stated in 1851, in my pamphlet "On a Remarkable Effect of Cross Breeding,"—that no trace or admixture of those antecedent male birds is ever seen in subsequent broods, the birds produced being in every instance as pure as in those in which no such crossing had been previously effected with the mother-bird.

It is but right, however, to state that two different individuals, writing quite independently of each other, have alleged that the reverse is the true state of the case. One of these is Mr. Orton of Sunderland ; the other, the reviewer in the *Aberdeen Journal* newspaper (February, 1851) of my pamphlet on cross breeding.

The latter, quoting my remarks on this point in the pamphlet, says:—

"He gives Mr. M'Gillivray's theory one advantage which is not its due. So far from it being the case that the peculiar effect of crossing here in question, fails to be seen in birds, it is in them that it is most of all apparent. It is well known that a hen-canary is rendered useless for breeding pure canaries by crossing for mules. After a cross with a goldfinch or linnet, no peculiar feature of these birds may turn up in subsequent broods by a male bird of the hen's own species, but every nest will be discoloured ; and so long as the hen produces young, it will be remarkable indeed, if a clean yellow canary be found among her progeny. This," he adds, "may easily be certified ; and we are inclined to think that it must be regarded, as in a great measure destructive of the theory of the purely physical cause of inoculation."

And so it would if true, but for the syphilitic test. This, it seems to me, places the theory in question beyond all doubt. But without speculating at present on the conclusions to be drawn from the fact, if the case be as this writer represents it, let us first consider whether it be a true or a "false" fact. Now, on reading the statement I have quoted, I sought and obtained a personal interview with the writer, and found that he could speak much less confidently than he had written, and, in particular, that he wrote from hearsay, and without any personal knowledge or experience in the matter. Having ascertained this I subsequently renewed the inquiries I had formerly made, with the same precaution as before to conceal the object I had in view, but among a larger number of

bird-fanciers, and with respect to a much greater variety of the bird-tribe. The result of this inquiry was, in every instance, the same as that of the former inquiry. None of my informants had ever seen anything that corresponded with the statement of the writer in the *Aberdeen Journal*.

More recently Mr. Orton, taking exception to the inoculation theory altogether, observes:—

"The hen does not carry her offspring *in utero*. Notwithstanding, her offspring, *as we have already seen*, are as liable to be influenced by the action under discussion (i. e., by crossing) as are those of mammalia."

And again—

"Hens and their offspring have not this uterine connection; consequently, they cannot undergo the process of inoculation laid down, yet they are, equally with mammalia, the subjects of this peculiar law of breeding." *

In the latter statement, Mr. Orton speaks absolutely; in the former, from an instance in point previously referred to, or adduced in some part of his paper. In all probability the *absolute* statement is founded on the *specific*, and to be judged of by it. Now, on referring to the instance which proves the ground of the latter, it is seen to be such an instance as would not be relied on, or regarded of any value whatever, in the case of a mammal. It is a case, not in which a hen-bird was bred from with a cock of her own breed, after having previously been bred from with a cock of a different species, but the reverse of this—a bantam hen having been bred from with a cock silk-fowl, and producing cross birds, after having been bred from the previous year with a bantam cock; and the observation relied on by Mr. Orton as fatal to the inoculation theory, was, that some of the birds of this brood bore a greater resemblance to the bantam, and less to the silk-fowl than did others. But this by no means warrants the inference, that the more decided resemblance of them to the bantam was due to the influence of the bantam cock of the preceding year. And I will venture confidently to affirm, to take a case in which a considerable number of young are thrown off at each litter, that, if a sow of a particular breed and colour be served with a boar of a widely different colour and breed, the sow never having been bred from before, the litter will be *mixed*; some of the young pigs taking chiefly after the boar, others of them chiefly after the sow. This test therefore, which may be called the *bird test*, if it cannot as yet be used in support of the inoculation theory, cannot at any rate be brought against it.

V. In my former papers on this subject of maternal inoculation through the fœtus, I referred in connection with it to a remarkable statement made by the excellent Count de Strzelecki,

* On the Physiology of Breeding, in the Newcastle Chronicle Nov. 10, 1854.

as to the effect of fruitful intercourse between the aboriginal female of certain countries and the European male, on the subsequent fertility of the female with a male of her own race. The count's statement, founded on "hundreds of instances" coming under his own observation, unqualified by the observation of a single exceptional instance, and bearing on the aborigines of Canada, of the United States, of California, Mexico, the South American Republics, the Marquesas, the Sandwich and Society Islands, and those of New Zealand and Australia, is, that "whenever such intercourse takes place, the native female is found to lose the power of conception on a renewal of intercourse with the male of her own race, retaining only that of procreating with the white men." *

Forbearing to trouble you with the application I made formerly of this statement, on the assumption of its being the expression of a general law of nature, to the theory under consideration—I think it right to mention with regard to the statement itself, that while I then suggested the propriety of "keeping the mean between the two extremes of too much stiffness in refusing, and of too much easiness in admitting" it to be a matter of *fact*, I have since received such information as warrants me in refusing to admit it as being *universally* true at least, in respect of certain of the races specified by the Count de Strzelecki. My friend and former pupil Dr. William Sim Murray, then of Her Majesty's 20th regiment of foot, writing me in November, 1851, from Montreal, where he was quartered, says:—

"Since my arrival here I have had an opportunity of speaking to Dr. French on the subject of the Count de Strzelecki's observation, and he tells me that, his attention being directed to it some time ago, he had made inquiries regarding it, and that he had obtained *positive* assertions that *squaws* frequently do have children by an Indian after bearing children to a European."

Again, Dr. Balfour, of Surinam, writing me in April, 1851, observes:—

"Living for many years in this district of South America, surrounded by a motley population of Whites, Negroes, and Indians, I have had very frequent opportunities of observing that Strzelecki's opinion of the effect of fruitful intercourse of the European male with the women of the aboriginal race (Indians here) is not borne out by the facts constantly occurring. The Indians of this district, known as the tribes of Warou and Arrawacka Indians, are certainly decreasing fast, but principally from the effects of that curse of the aborigines—*fire-water*. The women of these tribes are notoriously profligate during the earlier part of their life after puberty; but after bearing children to Europeans are frequently fruitful with men of their own race. This is particularly observed with the women of the Warou tribe, a camp of which tribe is in the immediate neighbourhood of the barracks, where a large detachment of European troops is constantly stationed. Any person resident in this district could at once refer to examples of this."

* Physical Description of New South Wales and Van Diemen's Land. pp. 345, 347.

And if I mistake not, a paper was published in the *Edinburgh Monthly Journal* in 1851 or 1852, in which the author, either writing from the antipodes or giving the results of his observations while there—whether in Australia or New Zealand, I do not now recollect—states that *exceptional* instances, at least, to Strzelecki's observation are of frequent occurrence among the aboriginal females of one or other or both of these countries.

I have thus endeavoured as impartially as one partial to the inoculation theory well could, to lay before you a complete general view of this interesting and important subject. I have referred to every fact known to me as bearing upon it, and have omitted nothing that seems in any way adverse to the theory in question. And submitting, in particular, the *bird test* and the *syphilis test* to the consideration of those among you who may take an interest in that theory, I will conclude by quoting a suggestion made by Dr. Carpenter in regard to the latter, but in its principle as applicable to the one test as to the other:—"As this is a point of great practical importance, it may be hoped that those who have the opportunity of bringing observation to bear upon it, will not omit to do so."*

Since the foregoing paper was written, and read to the Medical Society of Southampton, several important communications bearing on the subject of the inoculation theory have appeared in the *Medical Times and Gazette* and in the *Lancet*, from Mr. Hutchinson,† Mr. Savory, of St. Bartholomew's Hospital,‡ Mr. Langston Parker, of Birmingham,§ and Mr. De Méric, of London.|| I purpose considering these several communications in a future number of this journal, and will merely remark, meanwhile, that, by his well-conceived and admirably-executed experiments with strychnine, whereby he has demonstrated (what Magendie and Williams failed in demonstrating by theirs), that fluid matters do, in fact, pass from the foetus to the mother through the placenta, Mr. Savory has added to the chain of evidence in support of the inoculation theory, the only link wanting to make that chain complete. His discovery, for such it is, may be said to furnish the keystone of the arch on which the theory now rests, and rests securely.

* Principles of Human Physiology, 5th Ed., p. 826.

† "On the communication of Syphilis from the Fœtus to the Mother." *Medical Times and Gazette*, December 20, 1856, and January 10, 1857.

‡ "An Experimental Inquiry into the Effect upon the Mother of Poisoning the Fœtus." *Lancet*, April 10 and 17, 1858.

§ Lectures on Infantile Syphilis. *Lancet*, May and June, 1858.

|| Lettsomian Lectures—Lecture III. "On Hereditary Syphilis." *Lancet*, September 12, 1858.







